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THE 1980-81 AFOSR-HTTM-STANFORD CONFERENCE ON COMPLEX TURBULENT
FLOWS: COMPARISON OF COMPUTATION & EXPERIMENT.
UNCLASSIFIED FEB 82 SJ KLINE F49620-80-C-0027 AFOSR-TR-82-0371 NL

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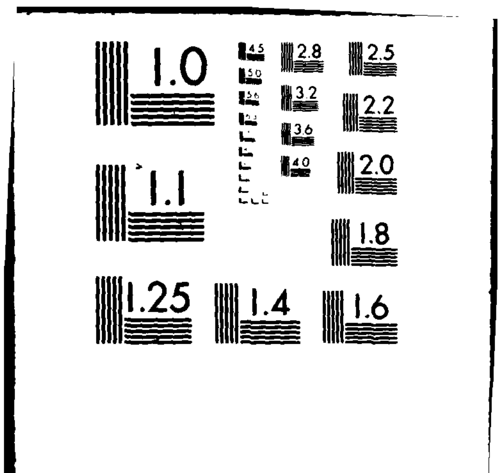

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AFOSR-TR- 82-0371

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Final Report
To
United States Air Force Office of Scientific Research
On
Contract AF F49620-80-C-0027

By

S.J. Kline
Principal Supervisor

Thermosciences Division
Department of Mechanical Engineering
Stanford University, California 94305

February 1982

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20. ABSTRACT (Continue on reverse side if necessary and identify by block number)

A summary is presented of the goals and results of the 1980-81 AFOSR-HTTM-Stanford Conference on Complex Turbulent Flows. The report of the Evaluation Committee for the comparison of computation with experimental data sets is included.

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Final Report

To

United States Air Force Office of Scientific Research

On

Contract AF F49620-80-C-0027

Covering work on
The 1980-81 AFOSR-HTTM-Stanford Conference on Complex Turbulent Flows:
Comparison of Computation and Experiment

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By

S. J. Kline
Principal Super

Thermosciences Division
Department of Mechanical Engineering
Stanford University, California 94305

February 1982

Chief, [illegible] Division

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ACKNOWLEDGMENTS

The principal financial sponsorship for the work of this conference was supplied by the U.S. Air Force Office of Scientific Research under contract AF F49620-80-C-0027 and a predecessor grant. Added support for work on data processing of compressible flow cases was supplied by the NASA Ames Research Center under Grant NAG 2-79. Contributions were also made by the Langley and Lewis Research Centers of NASA, by the U.S. Office of Naval Research and by the National Science Foundation. The specific assistance of Dr. Morris Rubesin and Dr. Dennis Bushnell of NASA with regard to funding is gratefully acknowledged. The steadfast support of Dr. James Wilson of AFOSR was critical to the success of the Conference. When the volume of cases found to be useful grew far beyond the initial estimates, with a resulting and considerable increase in cost, Dr. Wilson spent much effort to organize the concerned government agencies and thereby secure the funds to complete the work. Funds for some special purposes were also supplied from the Heat Transfer and Turbulence Mechanics (HTTM) Program of the Industrial Affiliates Program of the Stanford Thermosciences Division. Funds to cover an overdraft in the base contract were generously supplied by the Stanford School of Engineering.

I. Summary

The 1980-81 AFOSR-HTTM-Stanford Conference on Complex Turbulent Flows had three principal goals:

Establishment of trustworthy data sets that can be used as the basis for modeling complex turbulent flows and as standard trial cases for checking output of computation in such flows;

The creation of a data library in standardized machine-readable form on magnetic tape of the trustworthy data sets;

The comparison of current computational output from many groups with the standard trial cases.

The central report on the work is:

ALL THREE GOALS HAVE BEEN MET.

Some detailed remarks about accomplishments and the implications concerning the status of the field and for future work follows.

II. Comments on Some Specific Results

A. Data Evaluation

Some seventy* cases were recommended by "data evaluators" consisting of one or a few leading experts for the flow# in question and approved by the 1980 meeting.

Sixty-six of these cases were used in the 1981 meeting as test cases.

The process of evaluating data uncovered many important facts beyond the explicit goal of established trustworthy data sets. Among these are the following:

1. Improved dialogue between data takers and computors has been established. Each are more knowledgable about the others' needs, and this will aid orderly progress on research needs.

2. A paper, written serially by some six workers, mostly on the Organizing Committee, sets forth for the first time the "Data Needs" for computational fluid dynamics. Most older data do not really conform to these needs since they were taken for other purposes. The paper should assist future experimenters in the critical planning stages of their work.

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*Because some cases have subcases, the total is hard to count precisely, but seventy gives an appropriate impression.

#In the Conference nomenclature, a "flow" is a given class or geometry; a "case" is a specific realization of the class.

3. A number of long-standing problems in instrumentation and data control procedures were focused and discussed. A paper on uncertainty analysis (By R.J. Moffat) presents the first major conceptual advance in the analysis of uncertainty in single-sample experiments since the 1953 paper of Kline and McClintock. Moffat's paper provides a major improvement in our ability to insure control of experiments. The discussion during the 1980 meeting convinced a number of workers in the aeronautics community of the need to do explicit uncertainty analysis (Historically, Mechanical Engineers and Physicists had done this; Aeronautical Engineers had not).

4. The sections on "advices to data takers" in each flow provide very important guide lines to future experimenters concerned with creating high quality "record" experiments.

B. The Data Library

Under the able direction of Professor B.J. Cantwell, the data library has been created, and is in being as an ongoing activity. Funding at an appropriate level has been transferred to AFOSR Contract AF F49620-79-C-0010.

Tape II is in existence and can now be purchased as evidenced in the recent flyer announcing sale of the Proceedings of the Conference and of the data tapes. A copy of this "flyer" is appended as Attachment A.

Tape I was the first version of the data library and was furnished free to each of the participating groups. These groups were asked to send back to Professor B. J. Cantwell or Professor S. J. Kline any comments on format, errata, lacking symbols...etc. These comments were checked and when necessary corrections were made to Tape I. The corrected master from this process constitutes Tape II. Since 47 groups have used the tape, most errors should be eliminated.

We anticipate that Tape II will remain the standard reference for at least a few years. The precise date for updating to a Tape III has not been set at this time.

C. Evaluation of the Computations

1. Roughly 55 groups originally indicated they would perform computations. Some withdrew and others joined during the progress of the work. Ultimately, 47 computor groups submitted computations. A comparison with 1968 is instructive. In 1968 only one computor withdrew (owing to serious illness) from the 26 computors, and the work for that method was carried out by Stanford graduate students. The much larger withdrawal rate in 1981 speaks to the increased effort, costs and difficulty of current methods in CFD. Many groups also submitted significantly fewer cases than originally indicated in preliminary filings with the Organizing Committee; this attests to the same point; namely, the computation of the complex flows is by no means yet a routine, quick or simple matter.

2. An excellent, balanced report was submitted by the Evaluation Committee and is appended as Attachment B. The results of this report are not repeated since they speak for themselves. The Evaluation report provides a vastly improved understanding and overview of the state of CFD in 1981. It will be an important document in guiding future research. Comments on a few specifics are given in the following items 3-5 and general remarks in section III.

3. A central question noted at the opening of the 1980 meeting and written into the Introduction by S. J. Kline concerned whether or not a simple, single closure model could be found. The work of the Conference has largely settled that question. The possibility of such a model was not disproven, but the data suggest the liklihood of constructing such a model is extremely low. The arguments are stated in an Opinion by S. J. Kline; a copy is appended as Attachment C.

4. The work of the Conference also clarified a number of semantic and taxonomic points which should be of important aid in future research.

   a. During the organizing work it was noted that the word program was used in several ways, and that the use violated Russell's antinomy*. Thus, the Conference discussions and Proceedings speak of three levels: (i) a set of programs--all the methods used for solving the problems in hand by a given computor group (for example, airframe design); (ii) a program--the cards (or tape) loaded into a computer; (iii) a method--a single invariant computational procedure.

      This semantic clarification is important since some groups who had earlier indicated publicly that they had one program covering CFD were in fact using a program that was an assemblage of methods--a very different thing from the conceptual view and with very different implications for future researches.

   b. An important organization of the field was also achieved in the form of branched hierarchical descriptions including a short hand notation to describe each of the turbulent modeling procedures and the numerical methods employed. These taxonomies were in turn keyed to a taxonomy of the flows covered.

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*Russell's antinomy states that if concepts from different hierarchical levels are mixed, paradox will be created. See Principia Mathematica, A. Whitehead and B. Russell.

III. General Remarks and Future Actions

A. As both the Report of the Evaluation Committee and the Opinion by S. J. Kline indicate, the field of CFD has advanced enormously in the past decade, but much remains to be done. These two documents together not only clarify the state of the art, but also indicate an improved and probably more effective route toward the solution of further research problems in CFD. Thus, it seems clear that the 1980-81 AFOSR-HTTM-Stanford Conference is an important event and will have a major impact on research for some years to come. This fact is independently attested by the review of fluid mechanics events in Aeronautics and Astronautics covering 1981; this review is appended as Attachment D.

B. A remark on the philosophy employed in deciding the contents of the meeting and the Proceedings is appropriate. The philosophy adopted by the Organizing Committee was to make the contents "complete" in the sense of supplying all necessary elements--not just the comparisons of computation and data. Thus, a general introduction, a discussion of data needs, complete taxonomies of turbulence models and numerics, a description of the data library, problems of numerics, discussion of many general issues that arose during the conference by ad hoc committees and the special procedures for such committees and other elements were prepared. These will all appear in the three volumes of Proceedings.

The General Contents of the *Proceedings are shown* in Attachment A. Note, however, that owing to space limitation, Attachment A gives only main headings and thus does not fully describe some significant subsections.

C. On February 15, 1982, Volume I of the Proceedings is in press. Volume III is largely completed. Volume II is in editing. Volume I will be issued when printing and binding is completed. Volume III will be held and issued with Volume II since Volume III alone is not instructive. Several hundred orders for the volumes have been received and orders continue to arrive at the rate of a dozen or more per week.

D. Because the 1981 meeting did not arrive at specific recommendations on particular methods and because certain other very important questions remain open (for example, a method for differentiating numerical from modeling errors), the Organizing Committee recommended a follow-on meeting in 2-3 years. This meeting will be a far simpler task since the standard cases exist; the taxonomies exist; there is no need to repeat many items covered in ad hoc committee discussions; etc. The 1980-81 Conference clarifies the questions sufficiently so that the next meeting can be made more specific and sharply focused on critical issues.

Such a meeting is planned and will be chaired by Professor G. M. Lilley of Southampton University. Professor Lilley is an excellent choice for the chairmanship and is fully backed by the Organizing Committee. Several members of the Organizing Committee, including Professor S. J. Kline, will continue to serve in order to maintain strong continuity.

IV. Closing Remarks

In sum, the 1980-81 AFOSR-HTTM-Stanford Conference has been an enormous effort, possible only with the full cooperation of a major fraction of the turbulence research community internationally. Since the results will play an important role in guiding other experimental and computational researches for some years to come, it has certainly been worth the effort.

ATTACHMENT A

THERMOSCIENCES DIVISION
STANFORD UNIVERSITY
MECHANICAL ENGINEERING DEPARTMENT
STANFORD, CALIFORNIA
94305

Complex Turbulent Flows

Computation
& Experiment

PROCEEDINGS

The 1980-81 AFOSR-HTTM-Stanford Conference on Complex Turbulent Flows:
The Comparison of Computation and Experiment

Three volumes of Proceedings and a Library of Data on Magnetic Tape are being prepared for sale by the Thermosciences Division, Department of Mechanical Engineering, Stanford University, California. An order form for the Proceedings and/or Data Library is on a separate sheet below.

The Conference was a three-year cooperative effort of a major fraction of the turbulence research community with three objectives:

1. To reach consensus in the turbulence research community on trustworthy data sets that can be used as input for turbulence modeling in complex flows and as a basis for standard "cases" for checking output of computations.

2. The creation of a Data Library on magnetic tape. The library will hold the cases certified through objective 1 in normalized, computer-readable form.

3. Comparison with output of current methods of computation for turbulent flows with test cases covering a broad range of flows in order to clarify the state of the art and provide improved insight for further research.

The Conference has met these goals as shown by the contents of the proceedings (overleaf). In addition the proceedings contain taxonomies of methods and of numerics that aid in understanding the complex and rapidly developing field of computational fluid dynamics (CFD). They also contain position papers on: the history and nature of the problem; data needs for CFD; a description of the data library; advances in the theory of uncertainty analysis and experimental control. The discussions in Volume I give many important suggestions for the improvement of future experiments for the flow classes covered, and summaries of the state of the art in several critical areas of instrumentation. The discussions in Volume II address the current problems of modeling including the state of CFD in 1981 and comments on universal versus zonal modeling that suggest fundamental changes in the philosophy of turbulence modeling for the future.

Contents of the Proceedings 1980-81 AFOSR-HTTM-Stanford Conference
On Complex Turbulent Flows: Comparison of Computation and Experiment.

Volume I:

Objectives, Evaluation of Data, Specifications of Test Cases, Discussion and Position Papers.

1. Introduction: History, Goals and Problems: S. Kline

2. Data Needs for CFD: P. Bradshaw, B. Cantwell, J. Ferziger and S. Kline with comments on Compressible Flows by M. Rubesin and C. Horstman.

3. The Data Library: B. Cantwell

4. Some Contributions to the Theory of Uncertainty Analysis: R. Moffat.

5. Pictorial Summary of the Cases: S. Honami, B. Cantwell

6. Summary of each case: criteria of selection, advices to future data takers, specification of test cases in standard form.

   Flows include boundary layers, secondary flows, wall jets, flow normal to a cylinder, separated airfoil, flow over backstep including cases with variable wall angles, duct entry, curved shear layers, mixing layers, wakes, transonic airfoils, shock boundary layer interaction, three dimensional shock interactions, supersonic flow over a cone at angle of attack, supersonic flow over a cavity with reattachment, and others.

7. Discussions include points of agreement and disagreement among leading experimentalists on each case.

Volume II:

Taxonomies, Methods and Conclusions.

1. Taxonomy of methods in hierarchical, branched form: J. Ferziger, J. Bardina, J. Cousteix, B. Launder, W. Rodi, K. Hanjalic.

2. Taxonomy of numerics including methods of discretizing and methods of solving: J. Ferziger, G. Allen. Report on tests of Numerics with a given $K-\epsilon$ model. B. Launder and others.

3. Summaries by Reporters on classes of flows.

4. Summaries of experiences in computing by each of the 47 contributing computor groups covering 73 methods of modeling, and added comment by a committee of 17 groups.

5. Report of Evaluation Committee (H. Emmons, Chairman, D. Chapman, P. Hill, G. Lilley, M. Lubert, M. Morkovin, W. Reynolds, P. Roache). An evaluation of the state of the art and future needs.

6. Universal or Zonal Modeling: The Road Ahead. An opinion on directions for research: S. Kline

7. Use of CFD in Industry: E. Tjonneland and G. Sovran

Volume III:

Plotted Comparison of Output for All Computations with Data in Standard Form--By Cases.

O R D E R   I N F O R M A T I O N

Proceedings and Data Tape

1980-81 AFOSR-HTTM-Stanford Conference on Complex Turbulent Flows:
Comparison of Computation and Experiment

Complete Set of Proceedings, Three Volumes

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Any Single Volume	$ 50.00/Volume

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Volumes II and III, Spring 1982.

All prices include shipping costs, book rate, in the United States, Canada and Mexico.

Outside the U.S., Mexico and Canada add for:

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Data Library (Tape I)	$ 1.66
Air Mail: Single volume	$ 25.00
Set of three volumes	$ 75.00
Data Library (Tape I)	$ 23.83

Orders for delivery within California add 6-1/2% sales tax to cost of volumes ordered.

P.S. You can also order the 2 volume set of the proceedings of The 1968 AFOSR-IFP-Stanford Conference on Computation Turbulent Boundary Layers using the enclosed order form.

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___	Volume III	$50.00/volume	____________

___ Data Library on tape $150.00/tape ____________

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ATTACHMENT B

THE 1980-81 AFOSR-HTTM-STANFORD CONFERENCE ON COMPLEX TURBULENT FLOWS: COMPARISON OF COMPUTATION AND EXPERIMENT

EVALUATION COMMITTEE REPORT

This conference, which is the second part of the complex turbulent flows meeting has presented turbulence modelers with a selection of the best experimental data on a wide range of turbulent flows--66 cases in all. Forty-seven different computor groups used 73 different methods. This resulted in 1266 curves comparing calculation results with experimental data.

This tremendous computational effort supplies a great deal of detailed information which will take some years to digest--and additional calculations--to fully understand.

The Evaluation Panel with nine man-weeks of total effort has been able to do no more than discern certain major features of the status of our 1981 computational capabilities. We invite all present and future computors to study the results of this conference comparisons as an aid to the important areas to be further developed.

A comparison of the results of the 1968 and 1981 turbulence conferences shows the considerable increase in capability. The most remarkable increase in capability is in the range of turbulent flows that can be calculated with some degree of success.

In this conference significant progress was shown in the calculation of separated flows, boundary-layer shock-wave interaction, calculation of the various turbulent velocity components u', v', w', the decay of turbulence toward laminar flow, and transonic flows. Both elliptic and parabolic problems can now be done with fair success. In 1968 there were only a few programs based upon field equations with a one-equation model. There were no two-equation models. We now have a much wider range of models with a wide range of applications.

The increase of capacity in available calculating machines over the past decade has been remarkable and, as is expected, the turbulence calculators have expanded their desires and efforts correspondingly. Thus, some of the problems calculated for this 1981 Conference could not have been attempted in 1968, even if the physical models had been formulated at that time.

Having noted all these important advances, we must not suppose that there is nothing more to do. In fact there are no flows and no methods which are wholly satisfactory. Indeed one of the test cases used in the 1968 Conference, the attached boundary-layer flow, has been also used in this Conference and the computed results are shown to be in about the same agreement with data as was shown in 1968.

In attempting to evaluate our present standing we recognize two areas of critical importance:

The mathematical model of the basic physics of turbulence.

The numerical technique chosen for its solution.

The mathematical models which use directly the Navier-Stokes equations are so demanding of computer memory and computer time on the largest computers that only special scientific studies of turbulence can be treated in this way, and then only at low Reynolds numbers. Higher Reynolds number can be achieved but only when some suitable approximation to the flow equations is introduced such as the "Large Eddy Simulation Method," or perhaps at some date in the future when computer capabilities have increased by some orders of magnitude. These are very important studies but are not of direct use in this conference.

The models considered are those represented by the taxonomy developed for this conference. The main groups are: integral models, one-equation models including Boussinesq models, algebraic stress models and multiple-equation models for the Reynolds Stresses.

The numerics affect the results of the current models both in the algorithm chosen and the number and distribution of nodal points and other computational details.

The result of any given computation depends intimately on both the mathematical model of turbulence and the numerical technique chosen. It is impossible in many of the flows computed to separate the physical from the numerical limitations of the present work. In a few cases--but only few--grid refinement or other tests for solution accuracy were used and even some of these still showed significant changes in the numerical results.

The Evaluation Committee therefore had to judge the current status of the computing of turbulence flows as a complex of model plus numerical status.

With this "global" view in mind we constructed a matrix of computations given as Figure 1. Figure 1 shows only the number of cases computed in each area. We in fact tried a simple quantitative evaluation of every flow calculation in its agreement with the specified data. This, an at best approximate assessment, is not included here because the cases computed were too sparse to give statistical significance to the results. Futhermore, it confirmed the impression gained during the week of presentations, that every method had its strong and weak points. No method had any significant universality. Likewise no method proved to be universally bad.

This table, together with our examination of individual flows and individual methods, have permitted us to draw some tentative conclusions.

				Classification of Method for Calculating Turbulent Flow.					
				One-point closure					Two-point closure
		Strain.	Flow Case (number)	Integral Methods	Prescribed eddy viscosity or ℓ	Boussinesq. (B)	Algebraic (A)	Reynolds Str. (RS)	3ED
INCOMPRESSIBLE	FREE SHEAR	Simple	0311/0321/0471	. / .	. / .	8 / 9	2 / 2	2 / 2	.
		Extra	0331/0332.	. / .	. / .	5 / 5	5 / 5	2 / 2	.
	WALL B.L.	Simple	0141/0211/0261/0612.	9 / 17	4 / 10	11 / 26	6 / 8	2 / 4	.
		Extra	0142/0143/0231/0232/0233/0241/0242/0244/0263	2 / 6	3 / 5	5 / 18	4 / 16	2 / 5	.
COMPRESSIBLE.	TRANSONIC	Simple (attached)	8601/8621.	5 / 5	3 / 3	2 / 2	1 / 1	. / .	.
		Extra (attached + sep^t)	8623/8611/8612/8691	1 / 2	. / .	1 / 1	2 / 2	. / .	.
	SUPERSONIC	Simple (attached)	8101/8201/8403/8411.	2 / 5	3 / 4	2 / 4	1 / 3	1 / 4	.
		Extra (a) (attached).	8501/8301	. / .	3 / 5	. / .	1 / 1	3 / 3	.
	SUPERSONIC	Extra (b) (attached + separated)	8631/8632/8651/8661/8663/8671/8641.	1 / 1	3 / 5	2 / 6	. / .	. / .	.
INCOMPRESSIBLE	External	Extra (stalled airfoil	0441	3 / 3	. / .	2 / 2	1 / 1	. / .	.
	B.L.	Extra (relaminar	0281	. / .	. / .	2 / 2	. / .	. / .	.
	INTERNAL	Extra (attached)	0111/0112/P.1./0511/0512	. / .	2 / 4	6 / 6	2 / 2	. / .	.
	INTERNAL	Extra (separated)	0421/0431/P2/P3.	3 / 4	3 / 4	11 / 14	3 / 3	3 / 4	.
INCOMPRESS-IBLE	Homogeneous	various	0371/0372/0373/0374/0375/0376	. / .	. / .	. / .	2 / 13	8 / 72	2 / 22

Number of Methods / No. of separate flowcases tackled

Figure 1 Matrix of Computations.

C-1. The most important conclusion is that all methods are well worthy of further study and refinement.

C-2. The weakest point of present one-point closure models is the ϵ equation. The computed results of many flows can be brought into good agreement with the data by tweeking the ϵ-equation constants (as ϵ itself). A better equation should make these changes in value an automatic part of the calculation. A corollary effect of the ϵ-equation defects is the too large length scale in adverse pressure gradients and near separation.

C-3. The use of the algebraic stress models would be expected to be better than a scalar Boussinesq method. In fact, in calculating certain flows, as the turbulence-induced secondary flows in a corner, the algebraic methods give a fair answer while Boussinesq methods give none. However, an evaluation of the flows presented in this conference show no significant difference on the average. The Evaluation Committee agrees that a significant difference should result as the algebraic methods are futher improved.

C-4. The methods which have included an integration to the wall have been somewhat better than those assuming a "law of the wall." Clearly a more general "law of the wall" could fix this discrepancy. However, we believe that the number of influences on the wall profile is so large and the computational capacity is so large that, except in various special cases, an integration to the wall is preferable.

C-5. The fact that none of the present methods are influenced by rotation of the turbulent flow is an indication that present models are deficient in this respect. This question needs further study.

C-6. Present methods cause supersonic mixing layers to spread too fast. The repair of this defect should help guide further model improvement.

C-7. In turbulence models the simpler the treatment, the narrower the range of application. Thus we have a progression from the universally applicable (but impractical) full Navier-Stokes model to the narrowly applicable integral models. The progression is Full Navier-Stokes > Large Eddy Simulation > Reynolds Stress Models > Kinetic Energy and Decay Models > Mixing Length Models > Integral Models.

Model development and computer capability is slowly working up this list. However, this list is very significant in two additional respects: (1) The computation time in general decreases from FNS to I Models, so that for practical work an integral model is preferable if it is applicable. (2) Each of the easier models are based upon a greater amount of empirical knowledge. There is an important possibility of improving the simpler models by solving a series of flows by a more advanced method in order to determine appropriate empirical information for the simpler models. The more advanced model may in some cases be a better source of guidance than experiment, because the advanced model can supply information about quantities difficult or impossible to measure.

C-8. The fact that different numerics including algorithms and grid- and time-spacing yield different results was well brought out in the presentations, but was not adequately considered by the computors. We realize that halving the grid spacing is often computationally prohibitive on presently available machines. However, some testing of numerics is not prohibitive, especially for parabolic methods, such as use of higher-order accurate numerical methods for two-dimensional flows.

Although the results of the rough "quantitative" assessment could not be expected to produce statistically significant information on the relative successes of the computations in matching data, due to the small samples involved, nevertheless certain conclusions and trends were found and are reported here.

T-1. Before this meeting many workers in turbulents, fluid mechanics were probably of the opinion that Homogeneous Turbulent Flows could be calculated to a suitable accuracy if one or another of the more sophisticated turbulent-flow models were used. What came out of these calculations was that the Reynolds-stress models (RS) were only slightly worse than the two-point closure schemes (eddy damped quasi-normal hypothesis), but the results of the one-point closure scheme using an algebraic model were significantly worse. Of course, the task set computors was that of finding the time decay of the three (normal) Reynolds-stress components as well as the Reynolds shear stresses, and hence it was not surprising that one-point closure schemes using algebraic stress models were less than satisfactory. However, it was interesting to note the differences that came out of these computations and careful study of these results will no doubt bring forth some important information for turbulent-flow modelers, since they base many of their models on the results from homogeneous flows. The lack of success in these flows gives a measure of how far we are away from generating a universal turbulent flow model.

T-2. In the case of the flat-plate boundary layer in compressible flow with insulated and variable wall temperature, the results of all computations using a variety of methods showed a spread of results roughly equal to that of the experimental data. This spread of about ± 10% was probably higher than we had expected, and is of importance when we consider the corresponding accuracy which we can expect for calculations on more complex turbulent flows involving compressible flows.

T-3. The general accuracy of results involving separated flows was significantly worse than for corresponding attached flows. For a flow involving separation, the (RS) methods did no better than the less sophisticated approaches, and, in a restricted sense, the integral methods gave the best accuracy.

T-4. Similar conclusions to (3) were found for free-shear layers, where again the (RS) methods did no better than other methods, such as the algebraic (A) or Boussinesq (B) methods.

T-5. For attached boundary layers the RS methods showed some advantages over other methods, although again in a restricted sense the integral methods performed satisfactorily. For flows involving secondary flows the one-point closure methods gave useful results, but the methods in their present form are clearly in need of considerable refinement.

Although it is not possible to determine from the present results the relative merits of the various existing models, it is clear that this enormous computing effort has been well worthwhile in clarifying many fine details and in providing many specific tasks which, when added to the present results, will make the next ten years as profitable as the last ten.

Some of the suggestions which occur to the Evaluation Panel are listed for whatever they may be worth to the next ten years of computing effort:

S-1. One of the more important steps, which will make model comparisons more meaningful, will result from a more detailed concern for the numerical problems leading to accuracy of solutions. Only in this way is it possible to distinguish the precision of the physical modeling from the inherent numerical errors related to algorithm and grid. The effects of adjustment of a single model may be judged by use of poor numerics; different models cannot be safely compared. This is a very serious question deserving much greater consideration in the future.

S-2. The Conference has shown that many complex turbulent flows, incorporating both internal and external flows, can be computed to satisfatory engineering accuracy, although improvements and extensions are desirable and necessary. The design of codes for inexpensive engineering use often can justify less than perfect numerics. For this purpose codes should be approached as a package--a turbulence model with constants adjusted to give adequate results for a class of flows together with a narrow class of numerical methods and grid sizes.

S-3. It appears to the Evaluation Committee that more work is needed on the homogeneous flows by use of the Reynolds stress model. Success here could then cascade down through the simpler models to show the way to their improvement.

S-4. The models need special attention to the following:

a. the ε equation
b. the pressure-strain correlations
c. the effect of adverse pressure gradients
d. the effect of rotation
e. compressible flow mixing layers
f. detailed integration to the wall in some simple but adequate way.

S-5. The items under (4) above are open research questions. The answer may come from an ingenious analysis, trial and error, or simply a good guess. On the other hand, the best approach may in some cases be a carefully performed experiment. Every known technique should be tried as a guide to better future procedures.

S-6. It is clear that there are great difficulties involved in truly three-dimensional computations. However, the limited success of the three-dimensional computations presented to this conference is an encouragement for further efforts in this direction.

S-7. It may be desirable for a small computor group to examine the totality of results of the present calculations to locate those holes, where a few additional calculations would permit more definitive method comparisons, to select a few specific cases to be computed by everyone, and to hold a limited conference in a few years, which would give time for new computer codes and flow models to be tried and tested, and computed output to be available for comparison with the 1980 Data Bank. Such a conference could be held as a specific session at some national meeting.

If this is done, the Organizing Committee should require a detailed statement of not only the physical model used, but also the numerical method used, including plots indicating the exact number and location of the grid points. Also required should be an exact statement as to how and where the boundary conditions are satisfied.

The Evaluation Panel is aware of the fact that we are not able to do what we all wish were possible, namely to say this is better than that, so that work in the future be more narrowly directed. Nevertheless, certain flows such as the airfoil at transonic speeds and the airfoil at low speed at a large angle of attack in particular have been shown to be capable of flow prediction--features which were not thought remotely possible at the 1968 Conference. At the 1968 Conference, it was also felt that there was likely to be little interest in the future for integral methods once the two-equation and higher-order models had been further developed and improved. We see at this Conference that for the calculation of certain global features of turbulent shear flows, integral methods continue to perform adequately and for many engineering purposes are sufficient and preferable.

We have been most impressed by the great advances made since the 1968 Conference. We feel most encouraged for the future by the tremendous effort already expended and the spirit of cooperation, both betwen us and the computors, and the natural respect and coooperation between the computors themselves. We believe this conference provided an important push into the future in spite of few clear-cut evaluative decisions.

This report represents the consensus of the full Evaluation Committee

Members:

H. W. Emmons, Chairman

D. R. Chapman

P. G. Hill

G. M. Lilley

M. Lubert

M. V. Morkovin

W. C. Reynolds

P. Roache

J. Steger

ATTACHMENT C

UNIVERSAL OR ZONAL MODELING--THE ROAD AHEAD

A Personal Opinion By S. J. Kline

At the beginning of this meeting, I indicated that the central question to be considered at the current time was whether a universal closure model for turbulent flows has been or can be created, or, alternatively, whether it would be necessary to do what can be called "zonal modeling" in order to obtain results of engineering accuracy for practical flows in the near and intermediate future. This same question was addressed in a group of active researchers and government monitors at the NASA Langley Research Center in May 1980, and resulted in a sharp and relatively even division of opinions. At that time I abstained from the discussion since I felt it would have been inappropriate for me, as Chairman of the Organizing Committee for this Conference, to take a public position beforehand. The topic was also discussed at some length in response to the excellent questions prepared by Phil Klebanoff to begin the discussion in the dinner session concerning Session IV on Monday night of the present meeting. The question is a crucial one because it influences the central strategy of how one models turbulent flows. For all these reasons, I have been paying attention to the question as the computed results for this meeting accumulated, and during the discussions and presentations of the meeting. These added experiences have considerably clarified my own thinking. It therefore seems appropriate to discuss the topic of universal versus zonal modeling from several viewpoints, and then draw some personal conclusions.

The discussion begins with remarks on the general nature of models in physical science since that underlies the philosphy we employ. The arguments for a universal approach are then given. Next are sections discussing: (i) the functional nature of the Reynolds stresses needed for closure; (ii) the physics of turbulent flows; (iii) experiences with modeling prior to 1968, between 1968 and 1981, and finally the experiences of this Conference. The conclusions follow from these discussions.

When I was in high school, our science teachers told us that we were learning the universal laws of nature. In college I found that several central things my high school teachers had presented (such as the planetary picture of the atom and Newton's laws of motion) had already been overtaken by scientific revolutions* in some cases limiting the domain of applicability to something far less than "universal" and in other cases completely replacing the principle or concept with different improved models. However, it was not for another dozen years after my undergraduate work that

*In the sense of T. Kuhn

I came to the full realization that what my high school and university teachers had told me were the laws of nature were not that at all. They were rather, models made up by human minds to describe nature. Some of these models are of astounding elegance, breadth and accuracy; they are high pinnacles in the achievements of the human intellect. Nevertheless, they all are models with more or less breadth, but always with some limits of range of domain, and also with some residual uncertainties. At bottom, each of these principles, models or concepts is a truth assertion made by humans about nature.

Any single truth assertion of this sort is derived from and is intended to be true for a class of paradigmatic systems, and no more. There is a limit, in some cases very broad, and in other cases very narrow to the relevant class of systems. It is easy to prove this is so. One need only consider any truth assertion about nature whatsoever. We all know any such assertion can be made false, trivially, merely by changing the definition of the system. Once we clearly recognize that we are discussing human-made truth assertions about nature, and that all such assertions are inextricably tied to classes of systems our vision of the nature of these assertions is considerably sharpened. We see that the question is not whether a model is totally universal; none are adequate for all systems.* The relevant question becomes, "what is the domain over which this particular model gives adequately accurate predictions about nature?" That is precisely the question that confronts us in turbulence modeling.

The foregoing discussion makes clear that there is nothing "good" or "bad" about more or less universality of modeling. The important questions are pragmatic ones, "What works? What represents nature accurately?"

From this pragmatic view, it is clear that a universal closure model for turbulence would have advantages. It would allow turbulent flows to be modeled once and for all. It might be constructed more easily and quickly than a variety of models each fitted to special circumstances. Most important, it would provide us with assurance that we could do true PREdictive computations rather than merely POSTdictive computations with which we have long been familiar.

A universal model would also appeal to our sense of scientific fitness and elegance. But here me must be careful; the test is not elegance or seeming fitness, nor is it some subliminal desire to emulate Newton or Einstein. The only proper test is the pragmatic one, and our design needs demand we adhere tightly to the pragmatic test. There may be other aspects of universality that I have missed. I would

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*I can provide, on request, a recent more detailed study concerning the universality of the principles of physical science and their relation to more complex systems that defines the operational modifier "adequate" in this statement.

appreciate comments particularly from those who have or do take the position that we should focus on the search for a universal closure model. As an editor, I will be happy to include in the proceedings any substantive comments on this discussion that reach me from attendants at the 1981 meeting prior to 30 November 1981.

What is the functional nature of a complete turbulence closure model? When we time average the Navier Stokes equations, we lose information, and that information is inherent in the Reynolds stresses. The Reynolds stresses, for incompressible flow, are a second order tensor that is in general a multipoint function of four variables. Such a tensor is a complex quantity mathematically. We need to hold that complexity in mind for a moment in this discussion. We also need to hold in mind that for compressible flows we must deal with a number of different variables each of which has this complexity even if we use the simpler forms of the equations given by Favre averaging.

A geometric analogy may help our thinking. Suppose we imagine the terrain of a rough, glaciated mountain chain such as the Sierra Nevada of California or the Alps. Consider the nature of a model that might describe the topography: the peaks, valleys, spires, crevices, boulders, cirques, moraines and other features of such a mountain chain. Would we expect a simple algebraic equation with a few adjustable constants to describe such complex terrain? I think not. Nor would we expect a simple ordinary differential equation with one or a few adjustable constants to do the job.

How complex is turbulence as a phenomenon? Is it a relatively homogeneous terrain, or is it like the Sierra Nevada, composed of a very complex topography? Investigations over the past century answer this question quite well. The Introduction to Volume I of this proceedings contains a list of twenty-two quantities that each can significantly affect the nature of turbulence. These include not only pressure gradient, but various forms of wall curvature, body forces, additives and many other effects. Nor are we sure this list of 22 effects is yet complete. Contrary to earlier ideas, turbulence is not single or even a simple set of states; it is a very complex and variable set of states that react in sometimes unanticipated ways to a great variety of circumstances.

What we have learned about the structure of turbulence, mostly in the past 25 years, tells us the same thing. We can be quite sure that shear-flow turbulence is neither totally random not totally coherent; the available data effectively deny any such possibility. Shear-flow turbulence is rather quasi-coherent, or if you prefer, quasi-random, and such phenomena are inherently complex. Even more to the point for this discussion, we know that the quasi-coherent parts of the turbulent flows, what we call the large or medium eddy structures, are not the same in different kinds of turbulent flows. The structures observed in the near-wall region of attached boundary layers, that are so beautifully illustrated in the paper of P. Moin and J. Kim in this conference using Large Eddy Simulation, are quite different from those observed in free shear layers by such observers as Browand, Brown and Roshko, Bradshaw and many others. Work in the Stanford HTTM group has recently shown that the structure in the region near detattachment of a turbulent layer from a faired surface is distinctly different from that in the layer well before detachment--a point I will return to below. Nor does this exhaust the list of identifiable coherent structures characterizing particular flow zones.

What is our experience with universality in turbulence modeling? Before 1968, there was general disbelief that adequate models for turbulent boundary layers existed. The 1968 AFOSR-IFP-Stanford Conference showed that a number of adequate models for attached incompressible turbulent boundary layers did exist, provided: (i) they were not too close to detachment; and (ii) they were not reattached layers (Tillmann Ledge flow). Similar results were found in the NASA 1969 and 1972 Conferences for attached compressible layers and for the far zone of free shear layers. In the context of this discussion it is important to note two things: (i) <u>each of these three earlier conferences dealt with a class of flows with a single kind of flow structure</u>; (ii) <u>the modeling failed or was far less successful when we considered behavior beyond the edges of each class</u>. These difficulties were encountered near detachment, for reattached layers, and for the near zone of free shear layers.

What are the results in the 1980-81 AFOSR-HTTM-Stanford Conference. Several major results are relevant to the question of universality of modeling:

(i) No single model presented is accurate over the entire range of cases in this conference.

(ii) There is no correlation between sophistication (i.e., level) of model and accuracy of results over the full range of usable models.

(iii) Several times in discussions, individual computors reported success on some class followed by degradation of results when attempts were made to extend range of domain using a single <u>method</u>.* Much more evidence on this same point is evident in the fact that a number of groups explicitly shift method when moving from one class of flows to another.

(iv) If one looks over the total results presented by Computors in this meeting one finds one or more methods providing quite accurate results for nearly all of the flows tried. But the accurate method(s) vary from one case to the next. Moreover, the most accurate methods on several relatively

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*Method is used here in the sense of this Conference as an invariant procedure with fixed constants.

difficult cases are integral procedures; examples include: the transonic airfoil cases; the curved wall flows; and the planar diffuser of Simpson and his co-workers operating in transitory stall. This does not imply that integral procedures are more accurate or powerful; there are other cases where integral procedures give no output, and also cases where higher order methods have given better results.

The lesson to be drawn from the preceding paragraph seems clear. The evidence is strong that, at 1981, there is a definite trade-off between accuracy of output and the range of domain attempted. That was also true in 1968, 1969 and 1972, but was not so obvious or important because we studied single structural classes of flows. An example of this trade off is seen in the diffuser flows. Tthe most accurate method on the diffuser in transitory stall fails badly when used on the Pozzorini high-core turbulence diffuser flow since the method does not model the turbulence-turbulence interaction between the core and the boundary layer. This method does work particularly well in the detaching flow or transitory stall, however, in large part because it incorporates a specific, <u>different modeling</u> of the deattachment zone as contrasted with the fully attached layer. It is not a Prandtl-like two-zone method, but rather a three-zone method employing distinct models for the potential flow, the attached layer and the detachment zone.

Similarly integral methods specifically tailored to calculation of transonic airflows perform distinctly better in prediction of displacement thickness at the trailing edge than "more general" differential methods when applied to this problem. The differences in accuracy are of design significance as emphasized by Melnik.

The message seems to be clear. When we know enough about the physics, the structure of a given flow zone, (and often this is not a vast amount of knowledge), and if we systematicaly build this knowledge into our modeling, we obtain reasonably good accuracy. This seems true both for direct modeling of terms in model equations and for indirect modeling as in use of correlations in integral procedures. When we try to extend models even at the level of ASM or RST modeling to a very broad class of flows, at best we loose accuracy, and we may get quite inadequate results for specific purposes. It is not clear in the transonic airfoil cases if the poorer results of differential procedures arise from less accurate modeling or from loss of sufficient grid refinement with models this complex owing to computer size/cost limits. In the diffuser in transitory stall the situation is clear; it is improved modelling, more physical input, that makes the difference.

What does this say about the appealing idea of building universal models starting with the homogeneous cases to set constants and then systematically enlarging the range of domain without altering the constants set from the simpler flows? The evidence given above is not encouraging for this approach. We also recognize the fact that homogeneous flows have quite different structure features from shear flows. As noted by *Kline*[*] *the idea that homogeneous flows* extend by any kind of regular "expansion procedure" to shear flows is very questionable since homogeneous flows contain no production of turbulence and, therefore, do not provide a model for those features that control production. Moreover, the boundary conditions on homogeneous flows cannot, in general, simulate those for shear flows, and the boundary constraints are critical in determining structure.

What are we to do then? If we try to build separate models for various flow cases or applications, we face a hopeless task. There are, after all, a manifold of infinities of scientific flow cases, and a higher order manifold of infinities of geometries of commercial importance. The task would be endless. Fortunately, we do not need to model cases separately. In most flows of interest there are only a limited number of identifiable structural flow zones. By structural flow zones, I intend to denote a zone, a part or all of a flow field, that has roughly the same kind of flow structure. For the present I purposely leave the word "roughly" undefined. A tentative first cut at a list of such structural zones is given in Table 1 below; it contains twenty items. Several comments are crucial.

The list is not finished. It needs study and trials to see what works, to find what items might be consolidated, to see what needs to be added. We know a good bit about how to model many of these structural flow zones already. One need only look through the methods of this and the 1968, 1969 and 1972 Conferences. Certain items on the list delineate problems needing research (reattachment zones, shock/boundary layer interactions, recirculating zones for example). We also know a good bit about how to patch and/or match flow zones. In some cases, we need only a reasonable sliding of model constants, in others well developed techniques of asymptotic matching can be used. The computer has no particular difficulty in keeping track of where various zones lie throughout a computation; we know where we will need to patch or match. The various cases of complex strains and turbulent-turbulent interactions can be fitted into zonal modeling through treatment as subroutines in appropriate cases.

The idea of zonal modeling tied to structural flow zones is not new. It is in fact central to the famous 1904 paper of Prandtl, to the analysis of isentropic flow and shocks, and to many of the methods presented in this Conference. What has perhaps

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[*]IUTAM meeting Kyoto 1967 published as supplement to J. Phy. Fluids.

not been sufficiently emphasized is the importance of the tie between successful models and the physics--that is the structure features of particular flow zones. The important point is the following: It will almost certainly accelerate progress if we iterate turbulence models with experiments on structure, not in general, but rather for particular structural flow zones one by one.

Let us return for a moment to the functional viewpoint and the analogy of the mountain range. We know one accurate model for turbulence in Newtonian fluids; it is the three equations: (i) continuity; (ii) Navier-Stokes; (iii) the viscous energy equation--each in unaveraged form. As Peter Bradshaw has put it, God gave us one good model. Why should there be another model that is vastly simpler? We would not expect to find such a simple model for the topography of the Sierra Nevada, and nothing in the evidence cited above suggests turbulence is much simpler nor encourages the view that a simple, universal, turbulence mode that is not-too-slow and also adequately accurate can be found.

Given the review above, I have little faith that we will find a single, reasonably-fast, accurate turbulence closure model. The evidence suggests, to me at least, that the profitable road for engineering calculations in the near and intermediate future is systematic exploitation of zonal models tied directly to structure features of the flow. When we have done that in special cases, we have often succeeded. What remains now is to extend the approach to more general cases and to perfect the very important details.

The argument is sometimes made, as noted above, that zonal modelling will be more work and will, therefore, proceed more slowly than universal modelling. The argument seems plausible, but will certainly be untrue if no single, simple, adequate closure model exists. Nothing is slower than a search for the non-existent. Thus, I do not consider the conclusion I reach as discouraging. It seems to me quite the contrary. The argument suggests, apparently for the first time, what seems to be a feasible route toward achievement of what has eluded scientific research for more than a century. Let me put this differently. When we look for a single, not-too-slow closure model of engineering accuracy, we see failure not only in 1981 but many times before. In 1981 we do not even have a universal model for the known cases of homogeneous flows. When we look for not-too-slow methods of engineering accuracy for given structrual flow zones where we know enough of the physics, we see successes not only in 1981 but many times before. In 1981 we see successes of this sort for many more types of cases than in 1968; significant progress has occured. In 1981 some cases still are not well handled. For the most part, these are cases where we lack knowledge about the physics for one or more flow zones in the field and have tried to use models created for other types of flow zones. Examples include cases 0421, P2, P3, and 0411.

Before ending this discussion, I need to be clear on one more point. Nothing in this discussion is intended to suggest cessation of effort on higher-order or broader models for complex turbulent flows. The higher order approaches inform the lower order as the Evaluation Committee emphasizes. Computer power will continue to increase. In a decade much more effective use of RST models should be possible. We do, however, need to recognize more clearly that the zonal approach holds more promise than we have thought, and that design needs impel us toward development of the simpler forms of such zonal models in parallel with further development of higher order models.

APPENDIX I

KNOWN STRUCTURAL FLOW ZONES OMITTING HYPERSONICS AND WAVE PHENOMENA

1. Inviscid flow
2. Two-dimensional attached boundary layers
3. Three dimensional attached boundary layers
4. Reattaching/detaching zones
5. Mixing layer
6. Axisymmetric wake
7. Plane jet
8. Axisymmetric wake
9. Plane wake
10. Recirculation zone (fully stalled zone)
11. Shock/boundary layer interaction 2D
12. Shock/boundary layer interaction 3D
13. Mach No. effects on items 3 - 7 above
14. Secondary flow 1st type
15. Secondary flow 2nd type
16. Laminar boundary layers
17. Transition
18. Homogeneous flows
19. Trailing edge interactions
20. Large scale vortical motion

AEROSPACE HIGHLIGHTS 1981

Fluid Dynamics

Prepared by the Fluid Dynamics TC. Committee chairman, William G. Reinecke of Avco Systems Div.

A major event of the past year in both experimental and computational fluid dynamics has been the 1980-81 Conference on Complex Turbulent Flows, sponsored by the Air Force Office of Scientific Research with additional support from NASA, the Navy, NSF, and Stanford Univ. Recognizing turbulence—its understanding and prediction—as a key problem of modern fluid dynamics, a large group of researchers, organized principally by S. J. Kline and his colleagues at Stanford, met twice at the conference this year to compare computations and experiments in this field.

The 1980-81 conference continued and expanded the work of a similar conference on the computation of turbulent boundary layers held at Stanford in 1968. At that time, a standardized data set was chosen from the available experiments that had addressed incompressible turbulent boundary layer flows in a careful manner. Methods, then in their infancy, for computing these flows were tested against these experimental data. This 1968 exercise proved valuable in identifying trustworthy experiments and fruitful computational approaches, and in giving direction to future work.

The 1980-81 conference undertook a much more ambitious scope: not only turbulent boundary layers, but also a wide range of other complex turbulent flows. These included incompressible free and wall-bounded shear flows, homogeneous turbulence, separated flows, duct flows, and transonic and supersonic flows. In September 1980 a group of 160 researchers in fluid dynamics met at Stanford to reach a consensus on trustworthy experimental data sets that could be used for modelling turbulence in complex flows and as the basis for standard "trials" for checking the output of computations.

Based on the results of this meeting and a considerable effort by a group of data compilers led by B. J. Cantwell, a data library was established on magnetic tape. (Data tape available from Harold G. Hale, Jr., COSMIC, 112 Barrow Hall, Univ. of Georgia, Athens, Ga. 30602.) Sixty-six "benchmark" experiments have been included in this library, which is being held open for further additions and corrections.

The conference met again a year later, in September 1981, to compare the results of current computational methods for complex turbulent flows with the standardized results of the data library. Forty-seven groups submitted computations for this purpose. In a week-long meeting, computational methods ranging from integral techniques to Reynolds stress models were tested against the best available results from a wide range of experiments.

The report of the Evaluation Committee of the conference, chaired by H. W. Emmons of Harvard Univ., might be summarized as follows. Since 1968 there have been many important advances in flow modelling and in code development. As a result, many classes of external and internal complex turbulent flows can be computed to satisfactory engineering accuracy for a number of output flow variables. However, a number of gaps remain, for example, in modelling rotating flows and in the accuracy of certain model equations. Also, more complex flows, such as separated flows, are less well modelled at present than simpler cases. Moreover, there is no single model at any level of sophistication of closure modelling that handles well all the flows considered in this conference. For any given flow there is usually some method that provides adequate accuracy, but there is no correlation between the sophistication of the closure model and accuracy. For several important types of flows the most accurate method presented was a specially constructed integral procedure. Increased range of coverage and types of output militate in favor of more sophisticated models, but engineering design needs a push toward the use of the simplest method that will achieve a given output reliably. Therefore the evaluation committee concluded that the community should continue to pursue all levels of closure modelling actively.

These same results led S. J. Kline, near the end of the meeting, to state the opinion that we need to reconsider the universality of closure models. He believes that the practical road toward progress in engineering calculations in the near and intermediate future lies in zonal modelling tied to the underlying quasi-coherent structures. Kline noted the manifold geometric cases of industrial concern, but said only about two-dozen different turbulent flow structures need to be described. Moreover, the physics that requires modelling is in the flow structures and not in the geometries of the flows. Many of the most accurate computations in the 1981 meeting are of the zonal type.

These remarks reporting the conclusions of the Evaluation Committee and Kline are both necessarily highly abbreviated. The complete versions will appear along with the data, computations, taxono-

Class VI computer will replace Illiac IV, shown here, decommissioned by NASA-Ames. Boeing's new Class VI has already begun turning out solutions to fluid dynamics problems.

mies of flows and models, future data needs, and discussions from the meeting in the proceedings of the 1980-81 conference. The proceedings will be issued in three volumes and can be ordered from the Thermosciences Div., Dept. of Mechanical Engineering, Stanford, Univ., Calif. 94305.

The renewed interest in the solutions of the Euler equations (see AIAA Papers 81-0999 and 81-1259) has resulted from the need for a more accurate prediction of transonic flows when the local Mach number exceeds about 1.3. The approximation of neglecting vorticity production in curved shocks and entropy changes across shocks, as made in transonic potential methods, is not valid when shocks are strong. With further developments in the solution procedures of the Euler equations together with advances in grid generation and data management techniques, these equations will be routinely used in aircraft design and development processes.

The need for robust computational codes has led to the continued investigation of eigensystems composed of eigenvalues and eigenvectors of coefficient matrices of the Euler equations. An example of a method which makes use of local eigenvalues is that of MacCormack's 1981 semi-implicit scheme (AIAA Paper 81-0110). This method is very efficient compared to his 1969 explicit scheme. Another area which has continued to receive extensive attention is the multi-grid and multi-matrix methods, also called "multi-level" methods (AIAA Paper 81-1027). These investigations are important because they hold promise of obtaining steady-state solutions in a few sweeps through a mesh system, and as a result dramatically reduce the computatiuonal times. The present status of the multigrid method is typified by Ni (AIAA Paper 81-1025) for homoenergetic steady-state, transonic solutions of the Euler equations.

The discipline of computational aerodynamics is rapidly influencing helicopter rotor design. An illustration of rotor design with a computer code solving the full potential equation was first presented at the 1980 annual meeting of the American Helicopter Society (AHS Paper 80-2). This code was subsequently acquired by the helicopter industry. It is particularly useful in the study of rotor-tip designs at high advance ratios where the flow is transonic and strongly three-dimensional.

This year there have been significant trends in resources for computational aerodynamics. Since the middle of the 1970s the Illiac IV computer, the first of the Class VI computers, has been used to increase our understanding of the physics of turbulence and to compute pioneering solutions of the Reynolds-averaged Navier-Stokes equations. This year the Illiac IV was decommissioned. It will be replaced by a new and more cost-effective Class VI computer. The year also marked the entry of the aerospace industry into the use of supercomputers with Boeing's acquisition of a Class VI computer. By means of this computer the aerodynamicists Lee and Yu at Boeing were able, for the first time, to compute flow past a wing-body-nacelle-strut configuration with the transonic potential flow equation (AIAA Papers 81-0998 and 81-1258).

The trend toward large supercomputers, such as the Numerical Aerodynamic Simulator planned by NASA, was intensified by the strong effort of the Japanese computer industry with its government's support (*Business Week*, April 13, 1981).